When Money Runs Out: The Effect of Losing Grant Aid on Late-Stage College Persistence

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Qualifying Paper

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ABSTRACT

Although financial aid expenditures have grown considerably in recent decades, college dropout remains widespread. A natural question to ask is whether financial aid helps students progress to graduation. While many studies find that aid increases access and is worth the investment, much less is known about the effect on persistence, and in particular, the impact of aid on academic progress late into college. In this study, I examine the impact of need-based aid eligibility on late-stage college persistence by exploiting recent changes to federal Pell Grant eligibility rules that reduced the lifetime cap on aid from 18 to 12 semesters. Using eleven years of annual data from the October Current Population Survey and a difference-in-differences research design that compares income-eligible Pell students impacted by the rule change to income-eligible students not affected by the lifetime eligibility reduction, I find that eliminating Pell Grant eligibility decreased persistence late into college by 14-15 percentage points, or approximately 4 points per $1,000 of grant aid. This effect was concentrated among students who had completed several years of credits and were previously enrolled full-time at four-year institutions. The evidence in this study is therefore consistent with financial constraints posing a persistent barrier to educational attainment along the entire pathway to college completion.

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I. INTRODUCTION

For decades financial aid has been a widely utilized policy tool to support access to higher education and postsecondary attainment. In the fifty years since the passage of the federal Higher Education Act of 1965, average aid per student has more than tripled in real dollars, from $3,709 (in 2014 dollars) to $14,180, largely due to the expansion of federal aid programs (Baum, Elliott, & Ma, 2014; Dynarski & Scott-Clayton, 2013). Despite this growth in spending, many students who attend college withdraw before earning a certificate or degree. Less than one-third of degree-seeking students who enter community colleges earn an associate's or bachelor's degree within six years of initial enrollment and nearly 40 percent of undergraduates first attending four-year institutions exit without a degree.[1] Given the magnitude of dropouts, there is mounting concern that many students are leaving college without experiencing the full returns on their investment.

The size of aid expenditures and troubling outcomes have motivated questions about whether financial aid is effectively helping students progress to graduation. While causal research largely finds that aid increases access (Dynarski, 2003; Seftor & Turner, 2002; Turner, 2011), much less is known about the effect of aid on persistence, and in particular, its impact on late-stage progress to degree completion. By this I mean that little is known about whether aid that is disbursed beyond the second year of college supports progress to degree attainment.

In this paper, I shed light on the impact of losing eligibility for need-based aid on long-term college outcomes. Using a difference-in-differences research design, I exploit

[1] Authors' calculations using the U.S. Department of Education's National Center for Education Statistics 2004/2009 Beginning Postsecondary Students Survey.

recent changes to federal Pell Grant eligibility rules that reduced the lifetime cap on aid from 18 to 12 semesters beginning in the 2012-13 academic year. Because it immediately eliminated a subset of continuing students from receiving $3,600 in average grant aid, the rule change provides a source of exogenous variation that I use to estimate the causal effect of Pell Grant eligibility on persistence beyond the sixth year of college. This work therefore fills a gap in the literature on whether sustained investment in need-based aid could raise educational attainment levels. It also informs an important design consideration of grant programs, the length of time for which aid should be offered, which deserves attention because the majority of graduates take longer than the customary four years to earn a bachelor's degree.[2] In fact, nearly forty percent of undergraduates receive Pell Grants and nearly forty percent of Pell recipients take longer than six years to graduate (Baum et al., 2014; Wei & Horn, 2009). The findings in this study are therefore relevant to a large share of the college student population in the United States.

To preview my results, I find that losing Pell eligibility substantially increases the likelihood that income-eligible students leave college. I estimate that withdrawing Pell Grant offers to students enrolled for six or more years decreased persistence by 14.9 percentage points, or 24.7 percent relative to a mean persistence rate of 60.3 percent. I also find that this effect was concentrated among students who attended four-year institutions full-time in the previous academic year. Although the counterfactual returns

[2] The majority of students take five or six years to graduate for several reasons, including the fact that approximately 35 to 40 percent of them are required to take developmental education courses before they can progress towards degree requirements (Bettinger, Boatman, & Long, 2013). The majority of students also work while attending school, hindering full-time enrollment and slowing progress to completion (Bound, Lovenheim, & Turner, 2012; Scott-Clayton, 2012). Discontinuous enrollment is also widespread. Using administrative data from Florida and Ohio, I calculate that nearly one-third of students take time off for at least one semester before returning to pursue their degree.

to completion for students induced to dropout are not certain, prior research finds that finishing degrees produces an earnings boost for a variety of students and mitigates the risk of negative outcomes, including defaulting on student loans (Kane & Rouse, 1995; Looney & Yannelis, 2015; Zeidenberg, Scott, & Belfield, 2015; Zimmerman, 2014). The evidence in this study is therefore consistent with financial constraints posing a persistent barrier to educational attainment along the entire pathway to completion.

I structure the remainder of the paper as follows. In Section II, I discuss the theoretical motivation for this study, review existing evidence on the effects of financial aid on persistence and completion, and provide background information on the change to Pell Grant eligibility. I describe the data, analytic samples and research design in section III and present my empirical results in section IV. I conclude in section V with a policy discussion of the findings and directions for future research.

II. THEORETICAL MOTIVATION, PRIOR EVIDENCE & POLICY CONTEXT

Competing hypotheses posit that financial aid may help or have little impact on progress to degree completion. On the one hand, grant aid may persuade students on the margin of graduating to persist by reducing out-of-pocket expenses. For example, a multi-stage model of human capital investment predicts that students will choose to enroll in an additional year of college if the expected lifetime benefit of attending an extra year exceeds the expected lifetime benefit of dropping out (Bettinger, 2004). Because this model assumes that students update their expectations with experience, the decision to persist in college may be influenced by changes to the availability of tuition assistance. For students on the margin of completing college, losing aid may alter the cost-benefit evaluation enough to induce departure.

In addition to changing the investment value of attendance, students may become acclimatized to receiving aid, making it difficult to forecast and contingency plan for abrupt changes in funding. Older students may also face stiffer credit constraints than younger college-goers, and it may not be easy or possible for them to offset grant aid with additional student loans. All of these scenarios predict that losing eligibility for need-based aid will decrease the probability of early and late-stage persistence.

Yet for competing reasons, financial aid may have a diminishing effect as students near the finish line. For instance, students may become less likely to update their schooling decisions over time as their investments in college accumulate and the remaining cost to attainment declines. Decisions to persist may stabilize over time for this reason and attenuate the impact of aid on persistence as experience accrues. Losing aid may also have different short- and long-run effects depending on how much time students have to adjust. For example, if less aid comes as a surprise at first but is eventually predictable, the effect on persistence may be short-lived. Because grant aid offsets the full cost of attendance to students, it is also possible that offering aid late into college may delay graduation for some students.[3] Financial aid may therefore encourage extended "dabbling" that prolongs time to completion, while eliminating aid eligibility late into college may increase the efficiency of degree production.

Empirically, whether sustained investment in need-based aid facilitates progress to completion or not remains an open question. Although a growing body of evidence indicates that more generous need-based aid can increase enrollment, persistence, and

[3] In addition, the preference to delay completion may intensify in weak economic cycles when labor market opportunities for recent graduates are less certain. The aftermath of the Great Recession, which coincided with the elimination of late-stage Pell Grant aid eligibility, is one such period when financial aid might be expected to induce some students to forego graduation until more promising job opportunities arise.

degree completion at four-year colleges (Angrist et al., 2014; Bettinger, 2004; Castleman & Long, forthcoming; Goldrick-Rab et al., 2012), it remains unclear whether losing grant eligibility has a symmetrical effect on attainment. By focusing on the cumulative effects of aid over time, prior studies have also largely ignored whether enrollment duration moderates the impact on persistence and completion. In fact, I am aware of only one study that has addressed the time dimensionality of financial aid effectiveness. DesJardins, Ahlburg, & McCall (2002) find evidence that frontloading grants and scholarships would lead to modest increases in persistence at four-year college; however, the robustness of this finding is unclear since it is based on simulated policy changes. It is also unclear whether the result generalizes to the broad population of low-income students attending college today, given the authors' reliance on a dated sample of students enrolled at a single institution.

Extending the Literature to Late-Stage Persistence

In this study, I examine how losing eligibility for the federal Pell Grant affects the probability that advanced undergraduates persist in college. My findings extend the literature in two ways. First, I estimate average treatment effects which are more policy-relevant than the local average treatment effects reported in most available studies that rely on quasi-experimental methods (Castleman & Long, forthcoming; Goldrick-Rab et al., 2012; Marx & Turner, 2015). Second, I estimate effects for students who have made considerable progress towards a degree. As my review of the literature reveals, no studies have isolated the effect of aid on persistence as it is disbursed late into college. To the extent that aid increases completion, the effect may be driven by early subsidies that set students on a path to completion which they would follow in the absence of continued support. Alternatively, financial constraints may pose a formidable barrier to attainment along the entire pathway to completion. My findings tease out whether the effect of aid on persistence varies with time spent in college.

To investigate the research question, I exploit recent changes to Pell Grant eligibility rules that took effect in the 2012-13 academic year. In 2011, the Pell Grant program faced an $18 billion shortfall as a result of growing enrollments in college and recent program changes that made more students eligible for aid.[4] After infusing the program with $17 billion, Congress addressed the remaining funding gap by implementing four eligibility changes which applied to both incoming and continuing students:

1) Eliminating eligibility for students without a high school diploma or GED;
2) Eliminating eligibility for students who qualified for the smallest grant amount, equivalent to 10 percent of the maximum award, or $555;
3) Reducing the family income ceiling that automatically qualified students for the maximum award from $32,000 to $23,000; and
4) Reducing the lifetime duration of eligibility from 18 to 12 semesters.[5]

I examine the impact on persistence caused in particular by the change to the lifetime eligibility cap. Available estimates suggest that 60,000 – 100,000 undergraduates were affected by this rule change alone (Associate of Community College Trustees, 2011;

[4] The number of students receiving a Pell Grant increased by 13 and 27 percent in 2008-09 and 2009-10, respectively, whereas the year-over-year increase never exceeded 5 percent between 2004-05 and 2007-08 (Mahan, 2011). While part of this increase is attributable to lower opportunity costs of attendance during the Great Recession, Congress also relaxed income eligibility restrictions to qualify for Pell Grant aid and increased the maximum grant amount during this time, both of which contributed to skyrocketing program costs. Congressional Budget Office estimates suggest that approximately 10 percent of recipients in 2010-11 became eligible for aid because of these prior eligibility rule changes (Alsalam, 2013).

[5] Full-time equivalency provisions allowed part-time students to remain aid-eligible over a longer time horizon.

Institute for College Access and Success, 2011). There is also evidence that the rule disproportionately impacted students attending four-year institutions. For example, the California State University System predicted that 6,100 students, or 4 percent of its total undergraduate population, lost Pell eligibility as a result of the lifetime eligibility reduction (Nelson, 2012).[6]

The change to lifetime Pell eligibility coincided with another noteworthy program revision. In 2012-13, Pell-eligible students also lost access to year-round grants that allowed students completing a full credit load during the regular school year to receive a second award for summer attendance. In the 2010-11 school year, 1.2 million students (13 percent of award recipients) received supplemental awards that increased the average Pell Grant per recipient by approximately $200, or 6 percent (Baum et al., 2014; Delisle & Miller, 2015). Unfortunately, in my empirical analysis I am unable to isolate the effect on persistence of reducing the lifetime cap from the effect of eliminating year-long awards.[7] My results can therefore be interpreted as the combined effect of losing aid because of both program changes.

However, there are several reasons why the effects on persistence I find are likely driven by the reduction in lifetime eligibility. First, unlike the lifetime rule change, eliminating the year-round Pell did not affect aid eligibility during the traditional academic year. One might expect that losing year-round aid would affect time to degree

[6] Additional evidence that the rule disproportionately affected students attending four-year institutions is evident from the patterns of persistence across college sectors. While nearly 40 percent of Pell Grant recipients take more than six years to earn a bachelor's degree (Wei & Horn, 2009), only 10 percent of Pell recipients who began at a community college remain enrolled in the two-year sector after five years (Cho, Jacobs, & Zhang, 2013). The overwhelming majority of Pell recipients still enrolled in college after five years are therefore working towards bachelor's degrees at four-year institutions.

[7] Importantly, the simultaneous eligibility changes do not call into question the internal validity of my results, but rather the policy implications of my findings in regard to how need-based aid programs might be designed to support late-stage progress to degree completion.

completion for this reason, but not cause students late into their college careers to dropout altogether. Second, my research design is based on comparing the enrollment behavior of two groups of income-eligible students before and after the more stringent lifetime cap took effect, and both groups of students lost eligibility for supplemental Pell awards in the post-2012 period, while only one of these groups lost eligibility as a result of the lifetime rule change. Third, several reports indicate that the availability of year-long awards increased summer enrollment at two-, but not four-year institutions (Bannister & Kramer, 2015; Katsinas, Hagedorn, Mensel, & Friedel, 2011; U.S. Department of Education, 2011), whereas my results are based on a sample of individuals who enrolled at four-year institutions in the prior year and are driven by changes in their decisions to persist at four-year institutions.

III. DATA, SAMPLES, & RESEARCH DESIGN

Data

For my empirical analysis, I rely on eleven years of data from the 2004-2014 October Current Population Surveys (CPS). The CPS is a monthly, nationally-representative household survey collected by the U.S. Census Bureau and the Bureau of Labor Statistics. Each October, respondents are asked about the schooling behavior of all household members, and so I have information on whether all household members were enrolled in college, and if so, the type of institution they attended, their year in college, how many years of credits they have completed, and their enrollment intensity.

While the CPS is the best publicly available dataset for this study because of its national scope, detailed enrollment information, and synchronicity with the policy change, it does have some limitations. Specifically, I do not observe if individuals are eligible for the Pell Grant directly in the data. Following Seftor and Turner (2002), I use information on family income and household size to estimate the Expected Family Contribution (EFC) of individuals according to annual aid formulas published by the U.S. Department of Education (DOE). I use this EFC estimate, coupled with the average cost of in-state tuition at public institutions published in the Integrated Postsecondary Education Data System (IPEDS) and DOE Pell award schedules, to identify individuals who are likely income-eligible (i.e. Pell-EFC-eligible) for the Pell Grant in each academic year. Misclassification errors predominantly arise from the fact that asset information is not reported in the CPS. I therefore assume that all respondents have zero assets, which tends to overstate eligibility and will bias my results towards zero. The magnitude of this bias, however, is likely very small since most assets that families own (e.g. homes and retirement funds) are omitted from the aid eligibility formula and applicable assets count against eligibility only if they exceed a threshold.[8] In simulation work, Dynarski and Scott-Clayton (2007) find that the correlation of actual versus predicted Pell Grant amounts that excluded asset data was 0.95, and three quarters of applicants would have received exactly the same award amounts if assets were excluded from the award calculation.

The CPS is also limited because year of attendance is top coded in the data, so that individuals in their fourth year of college or beyond are collapsed into one group. I improve upon this limitation by matching individuals across years since half of the CPS sample in each year is surveyed again the following October.[9] This allows me to identify

[8] This threshold was $71,000 for independent students in the 2012-13 school year.
[9] This matching strategy would be compromised if factors correlated with college attainment, such as the condition of the economy, impacted the likelihood of mobility (and therefore match rates) over time. This does not appear to be a major threat to my results. The average number of matches per year in my main

all non-degree earners who would have needed to enroll in college for at least 5 years to earn a degree, and I treat this group as my proxy for students who exhausted their Pell eligibility as a result of the new lifetime limit.[10] In the remainder of the paper, I refer to this subset of individuals as the 5+ or treated group, and to those who completed fewer years of college as the 2-4 or control group. Since the 5+ group includes persons who enrolled in college and maintained their eligibility for aid after the lifetime rule change took effect, and also due to the measurement error in determining Pell-EFC-eligibility, my results likely provide lower bound estimates of the effect of losing grant eligibility on college persistence.

Samples

To examine the effects of aid on persistence, I condition the analytic sample on individuals who enrolled in college in the previous academic year. Because the lifetime rule change primarily eliminated eligibility for students at four-year institutions, I also restrict the sample to include only students enrolled at four-year colleges in the previous year. I further exclude individuals who earned a bachelor's degree between year t-1 and year t in order to isolate the effect on late-stage persistence. Lastly, I exclude 86 individuals whose highest level of education completed was less than a high school diploma and 102 individuals who would have qualified to receive a minimum Pell Grant award since those students lost aid eligibility as a result of the other rule changes

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sample is 1,109 and 1,042 before versus after the rule change, respectively. Among Pell-EFC-eligible individuals, the average number of matches per year in these two periods is 452 and 492, respectively.

[10] Through this approach, I rely on each individual's reported year in college in year t-1 to determine their expected year in college in year t. This strategy is defensible, given that very few matched individuals reported a year in college across surveys indicative of backtracking or leapfrogging. Ninety percent of all non-degree earners who enrolled at four-year institutions in subsequent years reported their class level in year t as being the same or one year higher than in the previous year.

Congress implemented in the Consolidated Appropriations Act of 2012.[11] After applying these restrictions, my sample consists of 4,540 non-degree earners who were Pell-EFC-eligible in school years 2005-06 through 2014-15.

In column 3 of Table 1, I report descriptive statistics for the Pell-EFC-eligible sample. For purposes of comparison, I also present attributes of four-year college undergraduates nationally in columns 1 and 2 using data from the 2012 National Postsecondary Student Aid Study and the characteristics of non-EFC-eligible individuals from the CPS in column 4. Approximately 59 percent of Pell-EFC-eligible individuals are female, which closely mirrors the profile of Pell Grant recipients nationally. Comparing the attributes of EFC- and non-EFC-eligible individuals within the CPS also indicates that the Pell eligibility indicator I created clearly distinguishes more- from less-advantaged students. The fraction minority is larger in the Pell-EFC-eligible group (40 percent versus 24 percent) and the mean income gap is nearly $68,000. However, on some dimensions the EFC-eligible students in the CPS sample appear slightly more advantaged than the overall population of Pell recipients. A smaller fraction of the Pell-EFC-eligible sample is minority (40 percent) relative to the census of Pell recipients (48.3 percent). Mean family income in the Pell-EFC-eligible sample is also $4,700 higher than the national average for Pell Grant recipients. To the extent that these differences reflect that the income-eligible students in the CPS sample are better equipped to absorb

[11] I keep individuals in the main sample whose award amounts were affected by the family income reduction that automatically qualified students for maximum awards, as nearly all of those students remained eligible for aid. A similar revision to the income ceiling in 2009-10, which increased the threshold from $20,000 to $30,000, suggests that the 2012-13 change reduced the average award per recipient by less than $250 (Alsalam, 2013). As a robustness check, I re-fit my statistical models after excluding these individuals and find that the change in auto-zero eligibility does not explain the overall findings.

financial aid losses than the typical Pell recipient, my results may further understate the average effect of losing aid eligibility on late-stage persistence.

Because my empirical strategy rests on making within-group comparisons between treated and control students before and after the lifetime eligibility cap was lowered, it is important that the relative composition of my sample remained stable over time. I examine evidence for this in Table 2. Columns 1 – 4 report group-specific means by years relative to the enactment of the lifetime rule change. In column 5, I present estimates of compositional changes to the sample over time. The coefficients on age indicate that the 5+ group of Pell-EFC-eligible students was slightly younger on average in the years preceding the rule change. None of the other estimates in column 5 are large or statistically significant, and given the small subset of 5+ students in the sample, it is possible that the age difference merely reflects sampling variability. Yet to the extent that it is not, a secular rise in age over time could upwardly bias my estimates since older students are less likely to persist in college on average.[12] As I discuss in more detail below, I account for this source of potential bias in my statistical models by controlling for the main effect of age and including interaction terms that allow the effect of age on schooling behavior to vary by class level and over time.

In addition to my main sample, I make use of two additional samples to explore the robustness of my findings.[13] The first incorporates the 5,395 income-ineligible individuals into the analytic sample to examine if my results are biased by secular

[12] In actuality, evidence suggests that older students were proportionately *more* likely to enroll in college in response to the Great Recession (Barr & Turner, 2013; Long, 2015). We might have therefore expected the share of older students to decline over time as the economy recovered. This provides additional evidence that the age difference among treated students may be an artifact of sampling error.

[13] I report summary statistics for the additional samples in Tables A1 and A2 in the Appendix.

enrollment changes between more and less advanced students.[14] The second includes students who earned a bachelor's degree between year t-1 and year t. Since my main sample is conditioned on non-degree earners, it is possible that I falsely attribute spikes in degree receipt leading up to the eligibility change to declines in persistence after the rule change took effect. Including degree earners in the sample allows me to investigate whether the probability of graduation increased in response to the impending loss of grant aid.[15]

Empirical Strategy

In my empirical analysis, I exploit the fact that the lifetime eligibility change impacted only Pell-EFC-eligible students with long enrollment histories beginning in the 2012-13 academic year. I therefore use a difference-in-differences (DD) strategy to estimate the average treatment effect of Pell Grant eligibility on late-stage persistence, where the first difference is whether or not a student would have enrolled in their fifth or higher year in college and the second difference is before versus after the lifetime rule change took effect. Under the assumption that the de-trended enrollment patterns for students in both groups would be the same in the absence of the eligibility change, I capture intent-to-treat effects of Pell aid eligibility on persistence by fitting the following statistical model:

$$(1) \qquad Y_{it} = \beta\, 5_i^{+} + \gamma\, Pre2012_t + \delta\, 5_i^{+} * Pre2012_t + \varphi_s \textstyle\sum_{s=1}^{2} Year_t^s \; + \\ \omega_s \textstyle\sum_{s=1}^{2} Year_t^s * 5_i^{+} + \varepsilon_{it},$$

[14] To partly account for error in estimating Pell-EFC-eligibility, I only include income-eligible individuals in the augmented sample who were ineligible in both survey years.

[15] I exclude 2-4 individuals from this sample because few students in the control group earned bachelor's degrees, and as a result, they do not provide a realistic counterfactual to compare against 5+ students. I instead use 5+ income-ineligible students as the control group in the analysis of degree attainment. The graduation sample therefore consists of 2,182 5+ students, of which 985 are Pell-EFC-eligible and 1,197 are non-EFC-eligible.

where Y_{it} is a measure of persistence for individual i in year t. $Pre2012_t$ is an indicator equal to one if the observation occurred before July 1, 2012 (i.e. individuals surveyed in October 2008 – 2011) and is zero otherwise, 5_i^+ is the treatment proxy that equals one if the individual completed five or more years of college and is zero otherwise, $Year_t$ is a continuous time trend which I model separately for 5+ and 2-4 individuals and allow to vary nonlinearly, and ε_{it} is a mean-zero random error term.[16] δ captures the intent-to-treat effect estimate of interest in this model. In all estimates, I report standard errors that account for the potential clustering of schooling behavior within households.[17]

To increase the precision of my estimates and examine their sensitivity to the inclusion of covariates, I successively introduce controls that build towards the following "full control" model:

$$(2) \qquad Y_{irt} = \beta\, 5_i^+ + \gamma\, Pre2012_t + \delta\, 5_i^+ * Pre2012_t + \varphi_s \sum_{s=1}^{2} Year_t^s + \omega_s \sum_{s=1}^{2} Year_t^s * 5_i^+ + \pi\, X_i + \theta\, X_i * 5_i^+ + \alpha X_i * Pre2012_t + \tau\, LMI_{rt} + \psi\, LMI_{rt} * 5_i^+ + \eta Rev_{st} + \lambda Rev_{st} * 5_i^+ + \kappa_r + \varepsilon_{irt}.$$

All common terms in equation (2) are defined as in equation (1). In this model, I also include a vector of individual-level covariates (X_i) comprised of indicators for sex, race, marital status, and whether or not the person resided in an urban area. This vector also includes continuous measures of age in years and household size. To allow for a different effect of these controls for treated and control students and to allow those effects to vary

[16] In Table A3, I present estimates from regressions that replace the linear and quadratic year terms with year and year-by-5^+ fixed effects. The results are substantively similar using this nonparametric specification, indicating that the linear and quadratic year terms closely approximate the functional form of the underlying time trend. However, because the parsimony from specifying a functional form buys more statistical precision, I present estimates from regressions that model time parametrically throughout my main results.

[17] The 4,540 individuals in my main sample reside in 3,999 unique households. The large number of clusters suggests that my estimates likely do not suffer from overstated precision, which I examine in more detail by calculating an alternative p-value after conducting a permutation test.

across time, I include their interactions with the treated indicator and with the pre-2012 indicator. I also include census division fixed effects (κ_r) that absorb all time-invariant regional differences in college attainment. In addition, because the lifetime rule change occurred amidst the recovery from the Great Recession, I also control for time-varying state and regional labor market conditions by including a labor market index that is an equally weighted composite of the state seasonally adjusted unemployment rate, state annual employment-to-population ratio, and regional job seekers-to-job openings ratio (LMI_{rt}), as well as a measure of annual state appropriations per student (Rev_{st}). Controlling for these factors mitigates spurious attribution of enrollment changes to the policy which could have been operating through distortions to the opportunity cost of college and institutional resource constraints over this time period. By including interactions of the labor market index and state appropriations controls with the treatment indicator, I also allow the effect of the economy to differ for treated and control students.

As a robustness check, I also fit a triple difference model (DDD) that uses the enrollment differences between 5+ and 2-4 individuals within the income-ineligible group as an additional control. This model, though underpowered, allows me to examine if the covariates in equation (2) do not fully capture secular changes that may have differentially affected the probability of re-enrollment between 5+ and 2-4 students over time. This specification takes the following form:

(3) $$Y_{it} = \beta_1\, 5_i^+ + \beta_2 5^+ * Pell_i + \gamma_1\, Pre2012_t + \gamma_2 Pre2012_t * Pell_i + \delta_1\, 5_i^+ * Pre2012_t + \delta_2 5_i^+ * Pre2012_t * Pell + \varphi_{s1} \sum_{s=1}^{2} Year_t^s + \varphi_{s2} \sum_{s=1}^{2} Year_t^s * Pell + \omega_{s1} \sum_{s=1}^{2} Year_t^s * 5_i^+ + \omega_{s2} \sum_{s=1}^{2} Year_t^s * 5_i^+ * Pell + \varepsilon_{it},$$

where all common terms are again defined as above and $Pell_i$ is equal to one for individuals who are Pell-EFC-eligible and is zero otherwise. In equation (3), δ_2 now represents the coefficient of interest: it estimates the difference in the difference-in-differences estimate between Pell-EFC-eligible and non-Pell-EFC-eligible students. I also augment equation (3) with the full set of controls in equation (2), and I additionally allow the effect of all covariates to vary by EFC eligibility status in this model.

IV. RESULTS

Preliminary Graphical Evidence

Comparing the re-enrollment trends of treated to control students suggests the lifetime rule change decreased persistence for students late into their college careers. In Panel A of Figure 1, I plot the fraction of Pell-EFC-eligible students that returned to college by class level and year. Among the control group, the persistence rate was relatively flat at around 0.76 from 2005 to 2014. By comparison, the trend for the treated group over this time horizon declined from 0.73 to 0.63 between 2005 and 2008 before rising in each of the next three years. In 2012, when the new lifetime cap first took effect, the persistence rate declined by almost 20 percentage points and remained near 0.60 thereafter. Variability in the persistence rate among 5+ students pre-2012 likely captures a combination of factors, including sampling error, the effect of the financial crisis, and prior eligibility changes to the Pell Grant program.[18]

In Panel B of Figure 1, I show the persistence trends of non-EFC-eligible students for additional comparison. The re-enrollment rates among this group held relatively

[18] See footnote 4 for a description of the previous program changes. Importantly, those changes endured after the lifetime cap reduction took effect. Therefore, while they may have contributed to the rise in persistence before 2012, it is unlikely that previous eligibility changes explain the precipitous post-treatment decline among treated students.

stable at around 0.80 and 0.65 for 2-4 and 5+ students, respectively, over the entire period. In contrast to the treatment group, 5+ students in the non-EFC-eligible group returned to college at almost the exact same rate immediately before and after the new lifetime rule took effect. Taken together, the divergent re-enrollment trends by treatment status suggest that losing grant eligibility decreased the probability of persistence among students who had invested considerably in their college education.

Difference-in-Differences Estimates of Effects on Persistence

I now investigate how much of the raw decline in persistence can be attributed to the policy change by fitting statistical models to the data. In Table 3, I present a series of estimates that control in various combinations for secular time trends, fixed student attributes and regional differences, and the strength of the labor market over time. Column 1 estimates equation (1) and includes only group-specific quadratic time trends as controls. Consistent with the descriptive graphical evidence, the coefficient on *Before* indicates no change in the probability of persistence for 2-4 students relative to when the lifetime rule change took effect. The coefficient on the interaction term in row 1 is the causal estimate of interest and implies that in the years preceding the rule change, eligibility for Pell Grant aid increased the probability of persistence late into college by 23 percentage points.

Of course, these estimates likely overestimate the true causal effect since equation (1) only accounts for time trends. Moving from left to right in Table 3, I therefore examine the sensitivity of the estimate to the inclusion of different controls. In column 2, I control for the gender, race, age, marital status, household size and residential urbanicity of individuals. I also include the interactions of these with the 5+ indicator and the pre-2012 indicator. This allows me to investigate if, as the results in Table 2 call into question, the effect on persistence is operating through a secular rise in the representation of older students within the 5+ group over time. The estimate in column 2 decreases slightly to 0.187 but remains substantively large and statistically significant. Likewise, when I control for the labor market index and state appropriations per student in column 3, the estimated effect on persistence is 0.211, nearly unchanged from the base model in column 1. In column 4, I estimate the full model as specified in equation (2). Jointly controlling for student demographics, regional effects, and the economic controls reduces the effect on persistence to 14.9 percentage points, but this estimate remains marginally significant at the 10 percent level. Off of a baseline persistence rate of 60.3 percent in the post-2012 period, this estimate implies that losing eligibility for Pell aid decreased persistence among 5+ students by 24.7 percent.

The typical enrollment patterns of students late into college suggest that the overall effect on persistence should be driven by students withdrawing from four-year institutions and not transferring from four- to two-year colleges when Pell aid is lost. I examine whether this is the case in columns 2 and 3 of Table 4 by estimating effects separately by enrollment type.[19] Consistent with expectation, losing eligibility for Pell aid decreased the probability of persistence to a four-year institution by 14 percentage points (24.9 percent) and had no effect on two-year transfers. The results in columns 4-7 also indicate that all of the persistence decline post-2012 operated through students previously enrolled full-time at public colleges, perhaps because private four-year institutions are

[19] I also examined, but find no evidence of differential effects by race, gender, or dependency status.

more likely than public colleges to adjust student aid packages in response to the availability of federal tuition subsidies (Epple, Romano, Sarpça, & Sieg, 2013).

The fact that losing aid eligibility decreased the probability of persistence does not indicate whether the effect was short-lived or sustained over time. Indeed, it is possible that the eligibility change surprised institutions and families in the first year, but then had minimal impact in subsequent years when more lead time allowed for other sources of tuition assistance to offset the loss of grant aid. In Table 5, I examine the dynamic effects of the rule change in the first three years after the reduced lifetime cap took effect. The results provide little evidence that the effect diminished between 2012-13 and 2014-15. In column 1, the point estimates on persistence to any college in the first and third year post-rule change are -16.1 and -13.5 points, respectively. Although the magnitude of the estimate in year 2 is considerably smaller, I fail to reject that the effect is constant in each of the first three years following the rule change (p-value on F-test = 0.293). In column 2, I also show that the dynamic effect on persistence at four-year institutions, which ranges from a 12.6 percentage point decline in year 2 to a 15.2 point decline in year 3, is stable in the first three years after the new lifetime cap took effect.

Robustness Checks

I conduct several robustness analyses to validate that the loss of aid eligibility induced students late into college to dropout. First, I examine whether I overstate the precision of the estimated effect by conducting a permutation test. Following Garthwaite et al. (2014), I assign placebo treatments to each class level-by-year combination in the EFC-eligible and non-EFC-eligible samples. I then re-estimate equation (2) to obtain a distribution of placebo difference-in-differences estimates which I compare to the actual estimate in column 4 of Table 3. I present the results from this exercise in Figure 2, which shows that the actual estimate of 14.9 percentage points is larger in absolute magnitude than 94.4 percent of all placebo estimates. Furthermore, in Table A3 I report year-over-year difference-in-differences estimates for the actual treated group and show that the only significant effect is isolated to the year when the lifetime eligibility revision was introduced. Taken together, these results provide evidence that the actual estimate is capturing the effect of the eligibility policy shock and not random jumps in the persistence rate due to sampling variability from small class level-by-year cells.

As a second robustness check, I examine the stability of the persistence effect to alternative sample constructions in Table 6. In the first row, I reproduce the estimate from column 4 of Table 3. In the next five rows, I impose additional restrictions to the main sample. First, to examine if the effect on persistence is driven by the change to the income restriction which automatically qualifies students for maximum Pell awards, I exclude students with family incomes between $23,000 and $30,000 who were directly affected by that eligibility change. In the third row, I limit the sample to only the 2010 – 2014 school years to investigate if the main estimate is an artifact of previous program changes that were phased in from 2008 to 2009. The magnitude of the effect in both samples is approximately 16 percentage points and therefore robust to these exclusions. In the fourth row, I exclude states with the five largest public university systems to examine whether the effect on persistence is driven by changes to state-specific policies that coincided with the lifetime Pell eligibility revision. The magnitude of the estimate from this restriction is twice as large, indicating that the impact of losing Pell eligibility is not isolated to students from a particular geographic context.

In the last two rows of Table 6, I investigate if my results are sensitive to age restrictions. I first limit the sample to only 18-35 year olds, since approximately 12 percent of individuals in the main sample are younger or older students whose enrollment decisions may not be representative of college-goers more generally. The estimate (0.119), though imprecise, remains stable and thus implies that erratic attendance patterns among age-atypical students does not account for the decline in persistence of 5+ students post-2012. As a falsification test, I also re-estimate the difference-in-difference model after conditioning the sample on 17-24 year olds, since it is very unlikely that 5+ students in this age range would have enrolled in college long enough to exhaust their eligibility. As expected, the point estimate (.006) in this specification is near zero.

As a third robustness check, I estimate a triple difference (DDD) model using the enrollment patterns of non-EFC-eligible students as an additional control to examine if the main estimate is biased by secular changes not fully captured by the full set of covariates in equation (2). The results of this analysis are based on equation (3) and presented in Table 7. In column 1, I again reproduce the DD estimates for Pell-EFC-eligible individuals from column 4 of Table 3. Analogous results for non-EFC-eligible students are presented in column 2, which show no evidence that the change in lifetime eligibility affected the re-enrollment rates of aid-ineligible 5+ students. The point estimate (-.008) is near zero and opposite-signed. In column 3, I pool the Pell-EFC-eligible and non-EFC-eligible groups to fit the DDD specification. The coefficient on the triple interaction in row 1 is the causal estimate of interest, which, although estimated imprecisely, is larger than the DD estimate (0.161 versus 0.149).

I conduct a fourth robustness check to investigate whether the effect on persistence is confounded by conditioning the sample on non-degree earners. If students were aware of the lifetime aid limit leading up to the rule change, then it is possible that the impending loss of aid incentivized some students to graduate to avoid paying additional out-of-pocket costs to continue their studies. If this were the case, I could falsely attribute an attainment effect in the run-up to the rule change to a dropout effect after the new rule was implemented.

I examine the effect on bachelor's degree completion in column 1 of Table 8. In this analysis, I include students who graduated in year t-1 in the sample and investigate the probability of completion among only 5+ students. I again use a difference-in-difference design to estimate degree effects, where the first difference is before versus after the rule change and the second difference is now the difference in completion rates between Pell-EFC-eligible and non-EFC-eligible students. For this analysis, I also assign 2011-12 to the post-treatment period, for if students were incentivized to graduate more quickly, their motivation to do so would have emerged in the year before the change to aid eligibility. The results in Table 8 do not provide evidence that the effect on persistence can be attributed to bachelor's degree attainment. Although the estimate on degree attainment is imprecise and I cannot rule out the possibility of a large effect, the coefficient (-0.002) implies that losing aid had no effect on time to completion.

In addition to degree completion, I also investigate how much academic progress students made in college before they were induced to dropout. On the one hand, it is reasonable to expect that students who enrolled in college for several years would have completed a large share of their academic requirements. However, the relationship between time enrolled and academic progress is not clear-cut. For instance, the correlation between years in college and years of credits completed in my sample is just 0.68, and 58 percent of 5+ EFC-eligible students accumulated fewer than four years of college credits. It is therefore possible that the new lifetime rule simply prompted students with minimal credit attainment to withdraw. To the extent that this opened up enrollment opportunities for new students or increased the resources available to continuing students, the negative effect on persistence might be interpreted in a positive light.

In Figure 3, I examine the distribution of cumulative credits completed before and after the lifetime rule change for 5+ and 2-4 EFC-eligible students. Because the CPS captures credits completed as an ordinal measure (in years), I plot the fitted credit distributions for 2-4 and 5+ EFC-eligible students after estimating an ordered probit regression that includes the full controls listed in equation (2). The distributions are identical in the pre- and post-2012 periods for 2-4 individuals, while among the treated group there is a clear leftward shift in the credit distribution in the post-2012 period. In columns 3-7 of Table 8, I present marginal effect estimates of the change in the distribution of credits completed among treated students. The point estimate in column 6 indicates that the share of 5+ individuals completing four or more years of credits decreased by 17.7 percentage points (42.4 percent) after the lifetime cap was lowered. The estimates in columns 2-5 also indicate that the relative composition of treated students who completed fewer than four years of credits increased systematically after the rule change. The evidence in Figure 3 and Table 8 therefore indicate that the students induced to withdraw because of the rule change had completed a substantial share of the credits typically required for a bachelor's degree.[20] Collectively, the results of the robustness checks I conduct reinforce that the loss of grant eligibility decreased late-stage persistence.

V. CONCLUSION

The college dropout phenomenon is pervasive, and little to date is known about how late-stage financial aid affects persistence and whether the effect of aid on persistence varies along the pathway to completion. Using a difference-in-differences research design, I exploit a recent eligibility change to the lifetime availability of federal Pell Grants to examine the effects of need-based aid eligibility on late-stage persistence.

My findings reveal that sustained investments in need-based grants are a necessary condition for maintaining academic progress for many low-income students. I find that losing aid eligibility after six years decreased persistence by 14-15 percentage points, or 25 percent, and that the entire effect was concentrated among students who had completed several years of credits and were previously enrolled full-time at four-year institutions. Based on the average Pell Grant award of $3,676 (in 2013 dollars) in 2011-12, my results imply that losing $1,000 in grant aid eligibility decreased late-stage persistence overall by approximately 4 percentage points. This estimate is consistent with previous studies that have examined the effect of need-based aid on initial enrollment and early persistence (Dynarski, 2003; Goldrick-Rab et al., 2014) and therefore suggests that the effect of need-based aid on enrollment is stable along the pipeline to completion.

[20] This interpretation is substantiated by two additional facts. First, analogous results for non-EFC-eligible individuals, presented in Figure A1 in the Appendix, show that the distributional shift in credits observed among 5+ students is isolated to the EFC-eligible group. Second, I find no evidence of initial credit completion differences (i.e. in year t-1) among EFC-eligible students before versus after 2012. Therefore, the distributional shift also does not therefore appear to be an artifact of compositional changes in prior academic progress among treated students.

Because this study is based on a sudden termination of aid, it is important to note that the findings may not generalize to other settings where aid is gradually reduced. Indeed, there have been many suggestions to frontload aid to help students establish their footing in college (Bettinger, 2004; DesJardins, Ahlburg, & McCall, 2002), and it remains possible that attenuating aid in moderation and with frequent reminder could have little consequence on postsecondary attainment. Nevertheless, because the effect on persistence I estimate shows no signs of fading out three years after the more stringent lifetime cap took effect, it remains unclear if students are capable of adapting to smaller award amounts without experiencing setbacks to college progress. It is also unclear if students would respond differently to frontloading policies according to the type of aid that is offered. These are important questions left for further research.

Although my results point to a large effect on persistence, I find scant evidence that the threat of losing aid accelerates time to degree completion. This implies, as in previous work (Scott-Clayton, 2011), that grant aid is a necessary, but insufficient condition for accelerating academic progress. From a program design perspective, my results imply that leveraging financial aid to accelerate time to degree completion likely requires offering performance incentives tied to attainment benchmarks, although how large those incentives must be to realize effects and the conditions of payment are not well-established. Furthermore, if the financial barriers to delayed completion are of second order importance, aid policy may have a limited role to play in increasing on-time graduation. Determining the cost-effectiveness of financial aid relative to other policy instruments (e.g. designing more structured degree programs, changing course enrollment defaults, intrusive advising, etc.) is a critical next step to addressing inefficiencies in degree production.

Beyond the implications to financial aid, my results point to the large role of policy shocks in student decisions to withdraw from college. In rational investment models of schooling decisions, students are assumed to update their expectations of college through learning and experience and withdraw when the expected costs of persistence exceed the expected benefits. Yet in light of recent evidence that many undergraduates leave college without a degree after making substantial academic progress, rational investment theory may not adequately explain the enrollment decisions of students in many circumstances (Bowen, Chingos, & McPherson, 2009; Mabel & Britton, 2016; Shapiro et al., 2014). My findings suggest that unanticipated hurdles may induce students to dropout not by choice, but out of necessity. Indeed, the results in this paper are consistent with previous research on the consequences of shocks which suggest low-income students may be at greater risk of dropping out because they have fewer physic and physical resources to anticipate and absorb unforeseen obstacles when they arise (Mullainathan & Shafir, 2013). Policy shocks may therefore play a larger role than is currently conceived in explaining overall levels of degree attainment and disparities by race and income. Understanding more broadly how shocks shape postsecondary trajectories and what investments buffer against them is important to resolving the gap between intention and attainment that characterizes the experience of many college students in the United States today.

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TABLES AND FIGURES

Table 1. Sample characteristics of college undergraduates by Pell status

	(1)	(2)	(3)	(4)
	NPSAS:12		CPS Analytic Samples	
	All 4-Year College Undergrads w/ Pell	All 4-Year College Undergrads w/out Pell	Pell-EFC-Eligible	Non-Pell-EFC-Eligible
Female	0.587	0.511	0.593	0.519
Age	25.10	23.30	24.98	25.28
	[0.12]	[0.15]	[8.00]	[9.23]
Married	0.162	0.116	0.190	0.169
Black	0.204	0.063	0.137	0.074
Latino	0.169	0.090	0.158	0.078
Other race	0.110	11.6	0.104	0.084
Family income	$30,326	$102,902	$35,036	$103,000
	[399]	[994]	[21,891]	[36,892]
Household size	3.20	3.70	3.79	3.69
	[0.02]	[0.02]	[1.63]	[1.29]
Urban residence			0.735	0.815
Re-enrolled in year t			0.751	0.785
Observations	4,346,400	4,879,200	4,540	5,395

Note: Means are shown with standard deviations in brackets. Columns (1) and (2) report statistics calculated with NCES PowerStats using NPSAS:12 sampling weights. Columns (3) and (4) report unweighted statistics.

Source: 2004-2014 October Current Population Surveys; 2012 National Postsecondary Student Aid Study.

Table 2. Sample characteristics of Pell-EFC-eligible students enrolled at a 4-year institution in the prior academic year by class level and years relative to the eligibility rule change

	(1)	(2)	(3)	(4)	(5)
	2005-2011		2012-2014		
	2-4 Yrs	5+ Yrs	2-4 Yrs	5+ Yrs	DD
Female	0.590	0.620	0.590	0.570	0.050
	[0.49]	[0.48]	[0.49]	[0.50]	(0.047)
Black	0.130	0.120	0.150	0.150	-0.016
	[0.34]	[0.33]	[0.36]	[0.36]	(0.033)
Latino	0.150	0.130	0.190	0.150	0.012
	[0.36]	[0.33]	[0.39]	[0.36]	(0.033)
Other race	0.100	0.080	0.110	0.130	-0.036
	[0.30]	[0.27]	[0.32]	[0.34]	(0.030)
Age	24.70	26.08	24.75	27.68	-1.541**
	[8.16]	[7.30]	[7.75]	[8.22]	(0.743)
Married	0.190	0.210	0.180	0.240	-0.046
	[0.39]	[0.41]	[0.38]	[0.43]	(0.039)
Household size	3.830	3.430	3.860	3.560	-0.097
	[1.61]	[1.63]	[1.63]	[1.81]	(0.165)
Urban residence	0.720	0.740	0.760	0.750	0.028
	[0.45]	[0.44]	[0.42]	[0.43]	(0.042)
Observations	2617	450	1274	199	

Note: Means are shown in columns (1)-(4) with standard deviations in brackets and estimates of compositional differences pre- and post-treatment reported in column (5). The estimates of compositional differences are from separate regressions. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 3. Difference-in-differences estimates of the effect of Pell Grant eligibility on the probability of persistence (N = 4,540)

	(1)	(2)	(3)	(4)
Before x 5+ Yrs	0.231**	0.187**	0.211**	0.149*
	(0.096)	(0.085)	(0.098)	(0.086)
5+ Yrs	-0.233***	0.029	-0.254*	0.015
	(0.074)	(0.111)	(0.151)	(0.162)
Before	-0.021	-0.059	-0.020	-0.058
	(0.033)	(0.075)	(0.034)	(0.075)
R^2	0.007	0.104	0.011	0.107
Mean of 5+ post-2012		0.603		
Controls				
Year Trends	✓	✓	✓	✓
Demographics (D)		✓		✓
D X 5+		✓		✓
D X Before		✓		✓
Labor Market Index (LMI)			✓	✓
LMI X 5+			✓	✓
State Appropriations per Student			✓	✓
State Appropriations X 5+			✓	✓
Region Fixed Effects			✓	✓

*** p<0.01 ** p<0.05 * p<0.10

Notes: Results are estimated with linear probability models. Quadratic year terms are modeled to account for non-linear secular trends and allowed to vary by treatment status. Individual demographics include indicators for gender, race, marital status, and urban residence, age in years, and household size. The labor market composite is an index of the seasonally adjusted state unemployment rate, employment-to-population ratio, and regional job seekers-to-job openings ratio. All models also include a constant. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 4. Difference-in-differences estimates of the effect of Pell Grant eligibility on persistence by re-enrollment type (N= 4,540)

	(1) Any	(2) Four-Year	(3) Two-Year	(4) Full-Time	(5) Part-Time	(6) Public	(7) Private
Before x 5+ Yrs	0.149*	0.140	0.008	0.158*	-0.010	0.153	-0.004
	(0.086)	(0.088)	(0.041)	(0.090)	(0.056)	(0.100)	(0.065)
5+ Yrs	0.015	0.110	-0.095	0.152	-0.137	-0.093	0.108
	(0.162)	(0.170)	(0.076)	(0.173)	(0.123)	(0.183)	(0.113)
Before	-0.058	-0.021	-0.037	-0.016	-0.042	-0.004	-0.055
	(0.075)	(0.081)	(0.050)	(0.078)	(0.051)	(0.083)	(0.060)
R^2	0.107	0.098	0.029	0.156	0.039	0.061	0.042
Mean of 5+ post-2012	0.603	0.563	0.033	0.513	0.127	0.518	0.107

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models that include the full set of controls. See column (4) of Table 3 for details. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 5. Difference-in-differences estimates of the effect of Pell Grant eligibility on persistence by year following the reduction to lifetime eligibility (N = 4,540)

	(1) Any College	(2) 4-Year College
Year 1	-0.161**	-0.138*
	(0.070)	(0.072)
Year 2	-0.041	-0.126*
	(0.072)	(0.076)
Year 3	-0.135	-0.152*
	(0.086)	(0.086)
P-value on F-test: Constant Effects in Years 1-3	0.293	0.962
R^2	0.109	0.099

*** p<0.01 ** p<0.05 * p<0.10

Notes: Results are estimated with linear probability models that include the full set of controls. See column (4) of Table 3 for details. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 6. Difference-in-differences estimates of the effect of Pell Grant eligibility on persistence using alternative sample definitions

	(1) Effect Estimate	(2) Observations
Main estimate	0.149*	4,540
	(0.086)	
Restricted to students not affected by automatic-zero EFC rule change	0.161*	3,597
	(0.093)	
Restricted to 2010-2014 only	0.159	2,631
	(0.102)	
Excluding students in CA, GA, NY & OH	0.292***	3,619
	(0.098)	
Restricted to ages 18-35 only	0.119	4,013
	(0.090)	
Restricted to ages 17-24 only	0.006	3,200
	(0.099)	

*** $p<0.01$ ** $p<0.05$ * $p<0.10$

Notes: Each row reports an estimate from a separate regression. All results are estimated with linear probability models that include the full set of controls. See column 4 of Table 3 for details. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 7. Difference-in-difference-in-differences estimates of the effect of Pell Grant eligibility on the probability of persistence

	(1)	(2)	(3)
Model	DD	DD	DDD
	Pell-EFC-Eligible	Pell-EFC-Ineligible	Pooled Sample
Before x 5+ Yrs x Pell			0.161
			(0.126)
Before x 5+ Yrs	0.149*	-0.008	-0.014
	(0.086)	(0.092)	(0.092)
5+ Yrs	0.015	-0.057	-0.051
	(0.162)	(0.165)	(0.165)
Before	-0.058	0.007	-0.012
	(0.075)	(0.082)	(0.058)
Pell			0.045
			(0.082)
5+ x Pell			0.042
			(0.232)
Before x Pell			-0.048
			(0.043)
R^2	0.107	0.136	0.120
Observations	4,540	5,395	9,935

*** p<0.01 ** p<0.05 * p<0.10

Note: Results are estimated with linear probability models and include the full set of controls listed in column (4) of Table 3. The model in column (3) also includes a full set of interactions that allow all controls to vary by treatment-by-EFC eligibility status. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table 8. Difference-in-differences estimates of the effect of Pell Grant eligibility on the probability of bachelor's degree completion and the distribution of college credits completed

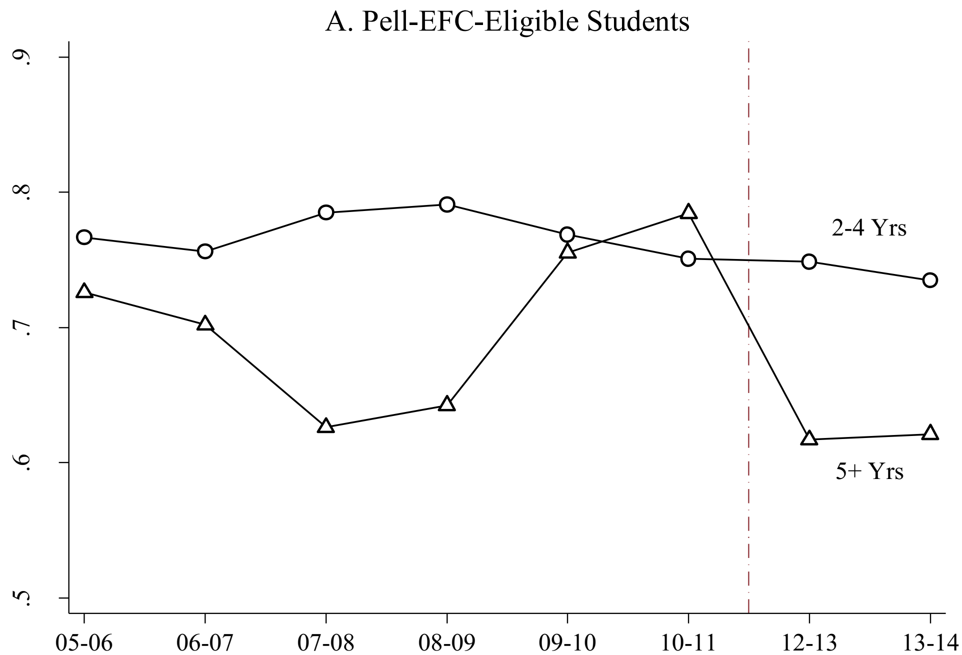
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	BA Degree	Years of College Credits Completed				
		< 1	1	2	3	4+
Before x Treated	-0.002	-0.018	-0.045	-0.101*	-0.012	0.177*
	(0.103)	(0.016)	(0.033)	(0.056)	(0.020)	(0.092)
Treated	-0.351*	-0.087***	-0.110***	-0.090*	0.124***	0.163**
	(0.187)	(0.021)	(0.034)	(0.050)	(0.025)	(0.079)
Before	-0.051	-0.003	-0.003	-0.000	0.004	0.002
	(0.074)	(0.015)	(0.013)	(0.002)	(0.018)	(0.011)
R^2	0.045					
Pseudo-R^2				0.060		
Mean of Treated in After Period	0.366	0.075	0.035	0.171	0.302	0.417
Observations	2,182			4,540		
Control Group						
Pell-EFC-Ineligible 5+ Students	✓					
Pell-EFC-Eligible 2-4 Students				✓		

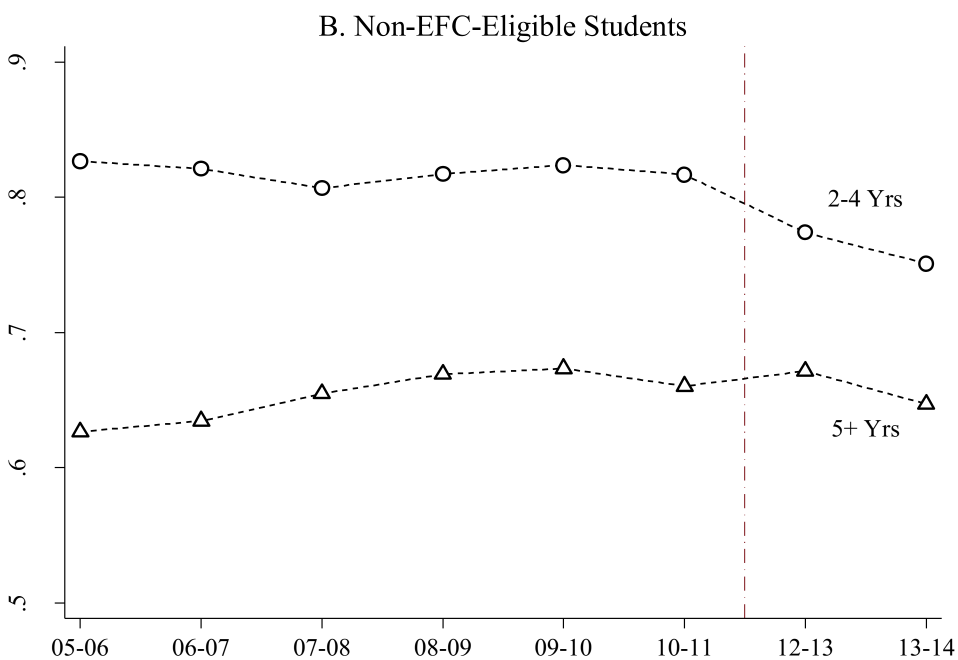
*** p<0.01 ** p<0.05 * p<0.10

Note: Results in column (1) are estimated using a linear probability model and include 2011-12 in the post-treatment period to account for the fact that any incentive to accelerate time-to-degree would have emerged in the academic year before losing aid eligibility occurred. Results in columns (2) - (6) report marginal effects on the probability of credits completed, and are evaluated at average covariate values after running an ordered probit model. All estimates are from models that include the full set of controls listed in column (4) of Table 3. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Figure 1. Fraction of undergraduates enrolled at a 4-year institution in the prior academic year returning to college by Pell EFC eligibility status, class level, and year

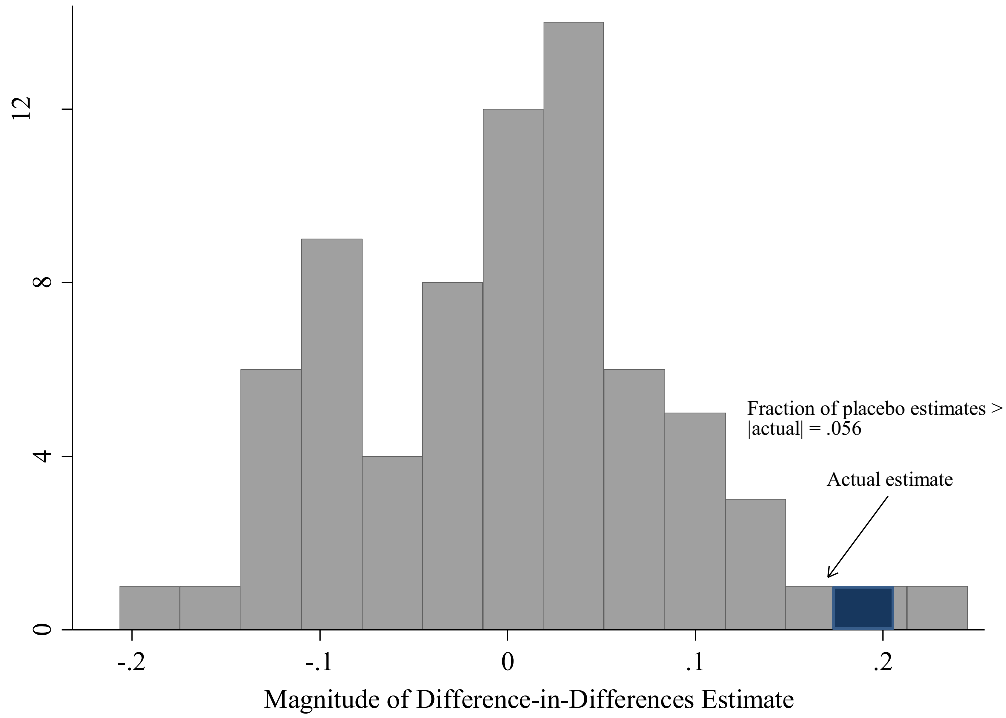




Note: Each point reports the two-year moving average. Reference lines denote the last year in which EFC-eligible students enrolled in college for 7-9 years remained eligible to receive a Pell grant.

Source: 2004-2014 Current Population Surveys.

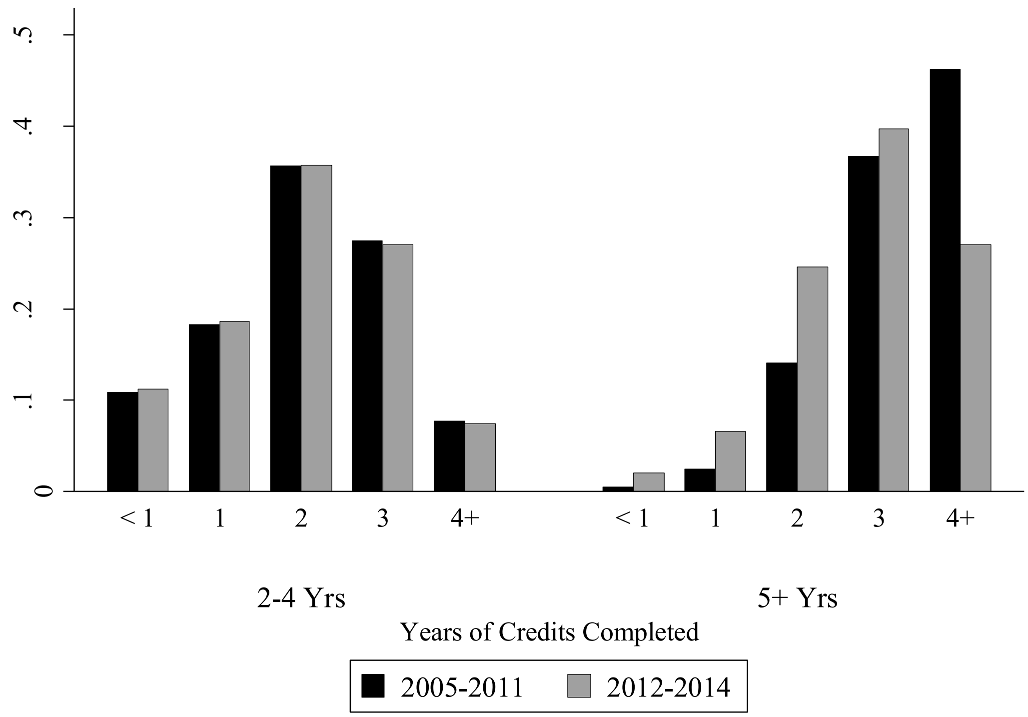
Figure 2. Distribution of actual and placebo difference-in-differences estimates of the effect of Pell Grant eligibility on the probability of persistence at a four-year college



Note: Each "effect" estimate is obtained after assigning "treatment" status to each class level-by-year combination in the Pell-EFC-eligible and non-EFC-eligible samples and then estimating a difference-in-differences model that includes the full set of controls. See column 4 of Table 3 for model details.

Source: 2004-2014 Current Population Surveys.

Figure 3. Fitted distributions of cumulative credits completed (in years) among Pell eligible EFC individuals enrolled at 4-year institutions in the prior academic year, by class level and before versus after Pell eligibility rule changes took effect



Note: The fitted credit distributions are estimated from an ordered probit regression that includes the full set of controls. See column 4 of Table 3 for model details.

Source: 2004-2014 Current Population Surveys.

APPENDIX

Table A1. Sample characteristics of non-EFC-eligible students enrolled at a 4-year institution in the prior academic year by class level and years relative to the eligibility rule change

	(1)	(2)	(3)	(4)	(5)
	2005-2011		2012-2014		DD
	2-4 Yrs	5+ Yrs	2-4 Yrs	5+ Yrs	
Female	0.53	0.48	0.52	0.46	0.017
	[0.50]	[0.50]	[0.50]	[0.50]	(0.044)
Black	0.08	0.07	0.07	0.06	0.010
	[0.27]	[0.26]	[0.26]	[0.23]	(0.022)
Latino	0.08	0.08	0.08	0.11	-0.022
	[0.26]	[0.27]	[0.27]	[0.31]	(0.027)
Other race	0.08	0.08	0.08	0.13	-0.045
	[0.28]	[0.28]	[0.27]	[0.33]	(0.028)
Age	24.73	29.30	24.73	27.56	1.746**
	[8.90]	[10.36]	[9.17]	[9.06]	(0.832)
Married	0.16	0.30	0.14	0.16	0.124***
	[0.37]	[0.46]	[0.35]	[0.37]	(0.035)
Household size	3.72	3.44	3.74	3.51	-0.048
	[1.29]	[1.30]	[1.23]	[1.38]	(0.117)
Urban residence	0.80	0.82	0.84	0.86	-0.006
	[0.40]	[0.38]	[0.37]	[0.34]	(0.032)
Observations	3438	523	1228	206	

*** p<0.01 ** p<0.05 * p<0.10

Note: Means are shown in columns (1)-(4) with standard deviations in brackets and estimates of compositional differences pre- and post-treatment reported in column (5). The estimates of compositional differences are from separate regressions. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table A2. Sample characteristics of bachelor's degree attainment sample by Pell EFC eligibility status and years relative to eligibility rule change

	(1)	(2)	(3)	(4)	(5)
	2004-2010		2011-2013		
	Pell-EFC-Eligible	Non-EFC-Eligible	Pell-EFC-Eligible	Non-EFC-Eligible	DD
Female	0.61	0.54	0.55	0.51	0.029
	[0.49]	[0.50]	[0.50]	[0.50]	(0.045)
Black	0.12	0.07	0.12	0.05	-0.005
	[0.33]	[0.25]	[0.32]	[0.23]	(0.027)
Latino	0.10	0.07	0.16	0.08	-0.046
	[0.30]	[0.25]	[0.36]	[0.27]	(0.029)
Other race	0.08	0.08	0.13	0.11	-0.027
	[0.27]	[0.27]	[0.34]	[0.31]	(0.029)
Age	25.35	28.09	26.71	26.76	-2.686***
	[7.18]	[10.26]	[8.54]	[9.36]	(0.814)
Married	0.20	0.28	0.24	0.17	-0.147***
	[0.40]	[0.45]	[0.43]	[0.38]	(0.038)
Household size	3.39	3.50	3.62	3.51	-0.214
	[1.67]	[1.39]	[1.77]	[1.33]	(0.148)
Urban residence	0.74	0.83	0.77	0.87	0.020
	[0.44]	[0.38]	[0.42]	[0.33]	(0.038)
Observations	635	811	350	386	

*** p<0.01 ** p<0.05 * p<0.10

Note: The sample includes individuals who earned a bachelor's degree in year t-1 and is limited to 5+ students. Means are shown in columns (1)-(4) with standard deviations in brackets and estimates of compositional differences pre- and post-treatment reported in column (5). The estimates of compositional differences are from separate regressions. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Table A3. Nonparametric year-over-year difference-in-differences estimates of the "effect" of Pell Grant eligibility on persistence (N = 4,540)

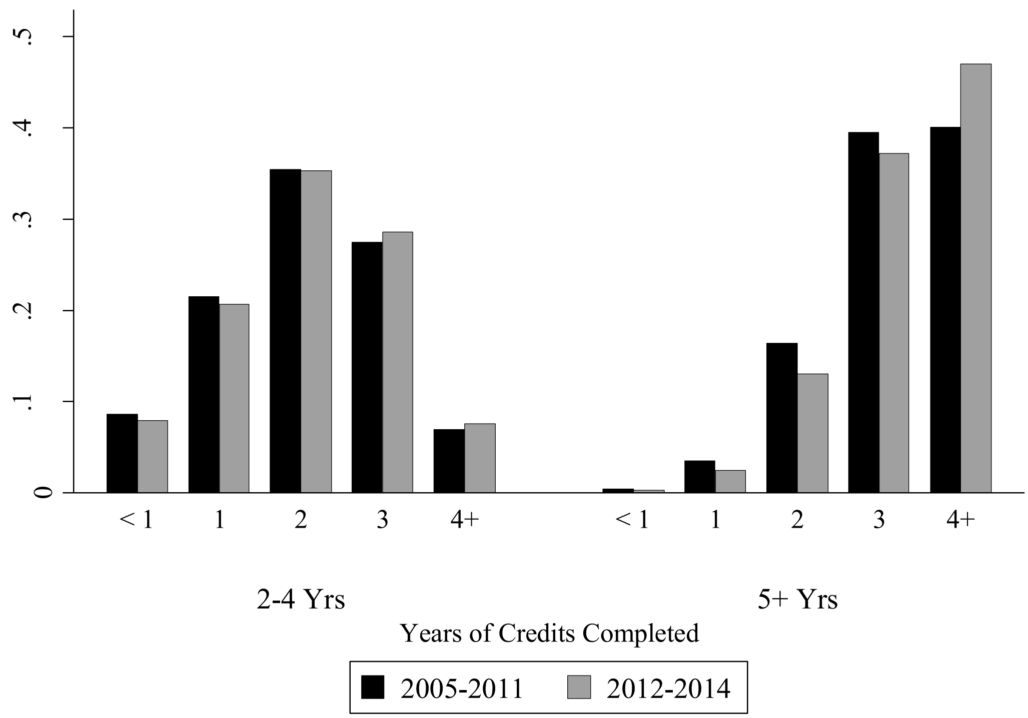
	(1)	(2)
Pre eligibility rule change		
2006 - 2005	0.046	0.027
	(0.089)	(0.084)
2007 - 2006	-0.081	-0.118
	(0.103)	(0.096)
2008 - 2007	-0.111	-0.117
	(0.104)	(0.100)
2009 - 2008	0.105	0.032
	(0.092)	(0.104)
2010 - 2009	0.119	0.129
	(0.084)	(0.084)
2011 - 2010	-0.002	0.020
	(0.070)	(0.066)
Post eligibility rule change		
2012 - 2011	-0.224***	-0.140*
	(0.081)	(0.072)
2013 - 2012	0.146	0.121
	(0.090)	(0.079)
2014 - 2013	-0.127	-0.089
	(0.096)	(0.090)
R^2	0.754	0.779

*** p<0.01 ** p<0.05 * p<0.10

Notes: Results are estimated with linear probability models. Column (1) includes year and year-by-5+ fixed effects. Column (2) also includes the full set of demographic and economic controls. See column (4) of Table 3 for details. Robust standard errors, clustered at the household level, are shown in parentheses.

Source: 2004-2014 October Current Population Surveys.

Figure A1. Fitted distributions of cumulative credits completed (in years) among non-EFC eligible individuals enrolled at 4-year institutions in the prior academic year, by class level and before versus after Pell eligibility rule changes took effect



Note: The fitted credit distributions are estimated from an ordered probit regression that includes the full set of controls. See column 4 of Table 3 for model details.
Source: 2004-2014 Current Population Surveys